pressing influence upon interspheral matter, and thus produce an elasticity, sufficient perhaps for the requirements of light radiation.

The lines of motion thus transferred through space cannot be unvarying in their orbital directions. Nature knows no great or small in her processes, and each moving particle of the free matter of space is controlled by the same principles which control the motions of a planet. It is subject to perturbations from lateral attractions, similar to those which draw planets and comets out of their orbits, and completely change the orbit of the latter. And its impacts with other particles yield effects such as would arise in impacts between planets of oppositely moving systems. Action and reaction are equal, in this as in every case. The orbit and the speed of a line of motion may be changed through impact or attractive resistance, but only by its causing an opposite change in some other line. Thus the lines of motor energy referred to are not unvarying in speed and direction, but are unvarying in their sum of correlated speeds and directions. The variations which take place in the orbits of spheres and comets through attractive perturbation, and the greater variations which would take place did spheres come frequently into contact, are precisely similar to those which must occur in the case of interspheral particles, and any change in the direction of one orbit is balanced by an equal opposite change in the direction of another orbit, the balance of motor direction and energy in nature being exactly preserved.

If such a line of motion pursues a cometary ellipse and enters the atmosphere of a globe, it must be affected by friction precisely as if the line of moving particles were a single particle, or a minute comet. It might be obliterated by friction or resistance, as the orbital motion of a falling body is obliterated. But this obliteration is really caused by the opposing energy of opposite lines of molecular motion. The single line of motion may be distributed into a thousand lines differing in direction, but the component of these thousand lines must agree with the original line.

The transfer of motion from particle to particle here indicated may take place through attractive resistance as well as through impact resistance. The original disintegration of the matter of space must have increased, as spheral condensation denuded space of much of its material, and as radiation from the spheres increased its motor energy. If matter thus divided up into smaller and smaller particles, these may have continued as closely contiguous in space as are the molecules of spheral atmospheres. In such a case they may present the conditions of excessive rarity so far as weight of matter is concerned; of close contiguity of particles, sufficient to permit the exercise of attractive energy; of great compression, through their vigour of centrifugal motion, and of intense elastic resistance to compression. These are the conditions necessary for the transfer of the radiations of light and heat. In these radiatures motion is conveyed through space by transfer of vibratory motions, not of impacts. The vibrating particle swings between lateral chains of attraction, and causes a like transverse swing in successive particles with which it is attractively connected. Greater energy here causes only greater width of vibration, not greater rapidity of transfer. The latter depends only on the elasticity of the matter concerned. Impact transfer of motion, on the contrary, must differ in speed with every difference in vigour. It is transferred by the motions of what we know as local heat, similar to the incessantly varied heat motions of gaseous matter. As the particles are unvarying in weight, increased momentum can be gained only by increased rapidity of motion, and the lines of motion thus transferred through space vary in speed with every variation in vigour. Every motion, of every particle of matter, is really a minute portion of an orbit, which represents that of a falling body, of a planet, or of a comet, according to its rapidity. Though the momentum affects successive particles of matter the orbit is continous, except to the extent that it is varied by perturbations through attraction and impact.

Wherever any influence aids a translation of interspheral matter—causes a wind to blow through space—the lines of motion continue to be conveyed by the same particles. The orbital motions of the spheres are such winds through space; minor aggregations of moving matter may enter the atmosphere of the sun or other globes. But no atmosphere can become permanently increased in this manner; such masses, checked by friction, must yield motion, which flows outward. The centrifugal energy of the molecules of the external atmosphere is thereby increased, and the gain of matter must be balanced by an equal escape of matter at that critical atmospheric limit where centrifugal and centripetal energies are in balance. But any such fall of interspheral matter must aid the radiant emissions of the sun. Its loss of proper motion, its high degree of absolute heat, its increased temperature through condensation, and its consequent radiation, would make it a source of solar heat. Any such cometary matter must form part of "The Fuel of the Sun."

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THE INSTITUTION OF MECHANICAL ENGINEERS

THE Annual General Meeting of this Institution was held on January 25 and 26, at the Institution of Civil Engineers, Great George Street; and the papers read were of unusual interest, from a scientific point of view, for a society whose aims are so distinctly practical. As it was pointed out by the president, Mr. Westmacott, three out of the five papers on the list were contributed by professors of science, and dealt with aspects more or less theoretical of the subjects treated upon. This forms, in fact, an additional instance of the way in which the old barriers between theory and practice are breaking down, and it is everywhere becoming recognised that neither can flourish without the aid of the other.

The first two papers, though quite independent, were both evolved, as it were, out of the same subject, namely, the research which the Institution has for some time been carrying on into the properties of hardened and unhardened steel. The first of these is an *interim* report by Prof. Abel, C.B., F.R.S., on the present stage of his experiments relating to the condition in which carbon exists in steel. Preliminary trials had shown that the treatment of steel and iron by a chromic acid solution (produced by mixing a solution of potassium bichromate, saturated in the cold, with one-twentieth of its volume of pure concentrated sulphuric acid) gave great promise of success in detecting the chemical differences existing in the same steel, according to the treatment to which it has been subjected. When cold—rolled, and annealed steel was thus treated, it yielded considerable amounts of an insoluble residue, consisting of black spangly particles, strongly attracted by the magnet, and presenting the characteristics of a true carbide, to which was assigned provisionally the formula Fe_6C_5. With hardened steel, on the other hand, but a small quantity of such particles were obtained, mixed with a lighter sediment; and the total residue contained only about one-sixth the carbon in the original steel, whereas in the annealed samples nearly all the original carbon was detected in the residue. The theory to which this points clearly is that in soft steel the carbon exists in a state of chemical combination, forming a carbide which is disseminated as a separate body through the mass of the iron; but that in hard steel this combination is dissolved, and the carbon exists in its pure form, either merely in mechanical admixture, as in the case of grey cast-iron, or in that peculiar and not very well understood form of association which metallurgists term an alloy. It would follow that the process of tempering, or rapid cooling, does not leave time for the complete formation of the carbide, and that in tempered steel all or some of the carbon still survives in its free or alloyed condition.

The fresh experiments described by Prof. Abel give, on the whole, great support to this theory. Four preparations were made of steel dissolved in chromic acid solution made as above, but of different degrees of strength. In the last only, where the strength was very high, were the results different, showing that the carbide had not been able to resist the oxidising effects of the solution. In the others, a considerable deposit was found, which, after being kept for several days, first in the original and afterwards in a fresh solution, was washed and dried, and then analysed. Another portion of the same was treated with chlorhydric acid, in order to ascertain what proportion would be converted into hydrocarbon. When this proportion was deducted from the whole, the remainder showed a most remarkable uniformity of composition, the percentages of carbon in three experiments being 5·93, 5·94, and 6·00 respectively. It seems evident that we have here a definite compound, to which Prof. Abel gives the formula Fe_3C. The deviations from this exact composition he accounts for by the presence of a certain amount of water, indicating that a carbo-hydrate had been formed probably as a result of the action on the carbide first separated.

Prof. Hughes's paper, which was illustrated by a series of very

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striking and elegant experiments, performed with the simplest apparatus, may be considered, in its result, as the complement of Prof. Abel's. The latter goes to show that in soft steel the carbon is present as a chemical compound, which is dissolved by hardening; the former, that in hard steel the carbon is present as an alloy, varying with the temper. Between the two, we seem to reach the threshold of a complete theory. They approach the subject, however, from different sides, Prof. Hughes's work being purely electrical, Prof. Abel's purely chemical; and this makes their convergence the more important. Finding that the induction balance was equally sensitive to molecular and to chemical changes, in the metals tested, Prof. Hughes set himself to devise an instrument by which to examine the former class of phenomena by themselves. A wire forming the core of an ordinary magnetic coil, and capable of being shifted, twisted, &c., as desired, supplies what he requires. The coil is joined to a galvanometer, or, better still, to a telephone; the wire is joined to a battery, and currents are sent through it. So long as the wire is at right angles to the coil, no effect is produced; but if we set it at an angle to the coil, sounds are instantly heard, betokening the presence of induced currents in the coil. This is the ordinary effect of electro-magnetic induction, as discovered by Faraday. Now, instead of shifting the wire, let us give it a slight twist, say of 40°: the sounds are instantly heard as before, and we detect induced currents, which are positive for right-hand torsion, negative for left-hand torsion. Prof. Hughes's explanation of this is that the molecules of the wire, which he regards as so many separate magnets, have been given a twist round the axis, and thus set at an angle to the coil, just as the whole wire was by the shifting in the first case. Now let us twist the wire still further, even to several complete turns. No greater strength of current is observed, showing that the angle once given to the molecules is not exceeded, and that the subsequent torsion is of the wire as a whole. Approach to the wire, thus twisted, one pole of a natural magnet, laid parallel to the wire: the sounds cease, indicating that the magnetism has spun the molecules back again into their original directions. Approach the magnet at right angles to the wire: the current returns to zero while it is still two inches distant, and when it is in contact there is a reversed current, which is then at its maximum. Lastly, removing the magnet, untwist the wire by some 40°: the current returns to zero, showing that the molecular torsion has disappeared, while the molar torsion remains almost the same as before.

In all these effects the wire has been supposed to be of soft iron: a remarkable difference appears when we turn to tempered steel. For we now fail to detect more than slight traces of molecular disturbance or rotation, no matter how many turns we give to the wire. Thus, whereas in the iron we appear to have great molecular freedom, with steel we have almost complete molecular rigidity. But this molecular rigidity is found to obtain also in all alloys of steel which have been tested—*e.g.* magnetic oxide, iron and sulphur, iron and tungsten, &c. Hence we draw the conclusion that tempered steel is likewise an alloy, the associated elements in this case being, of course, iron and carbon.

The above is the essential part of this striking paper; but the same idea of molecular freedom and rigidity was illustrated by other examples. Thus, if a tube nearly filled with iron filings be magnetised, the magnetism, though permanent so long as the tube is still, is removed in an instant by shaking, or even by turning the tube gently, so that the filings roll over each other. If, however, we pour in any viscous liquid, the magnetism can indeed be imparted, but it cannot be mechanically removed: the filings are no longer able to revolve back into their former positions. Again, we may magnetise an iron and a steel wire to the same degree, and then give each a slight pull to set it in vibration; it will be found that almost the whole of the magnetism has disappeared in the iron, while it is scarcely affected in the steel. By such illustrations the remarkable physical differences thus shown to exist between iron and steel were brought very clearly home to the audience; and whether they accepted or not the theoretical explanation, they could not fail to recognise the suggestive and practical character of the facts thus put before them.

The third paper on the list, by Mr. Chas. Cochrane, was a sequel to one read by the same author at the Leeds meeting of the Institution, and dealt with Blast-Furnace working, with special reference to the analysis of the escaping gases. It was laid down at the outset that economy of fuel in blast-furnaces is governed by three conditions: (1) the temperature of the blast; (2) that of the escaping gases; (3) the quantity of carbon which can be maintained in the condition, once attained, of carbonic acid, instead of being re-transformed into carbonic oxide by absorption of carbon in the fuel. On the first two of these heads there is, of course, nothing new to be said; but they were illustrated by elaborate and valuable tables, giving, in units of fuel (C burnt to CO), (*a*) the heat carried *in* by blast of a given weight and temperature, (*b*) the heat carried *out* by escaping gases of given weight and temperature. The third is dwelt on at some length; and tables are given, showing, for any given consumption of C per ton of pig, the ratio of CO_2 to CO in the escaping gases, first when all the CO_2, once formed, is retained in that condition; and afterwards when ½ cwt., 1 cwt., 1½ cwt., &c., are afterwards reconverted into CO, or, as the author terms it, when a *transfer* of ½ cwt., 1 cwt., &c., of C has taken place. From this is deduced the conclusion that the mere knowledge of the ratio of CO_2 to CO in the escaping gases, as given by analysis, is useless to indicate what is really going on in the furnace; because the same ratio may appertain to any different conditions, according to the amount of the transfer which has taken place, from CO_2 back to CO. If, however, the consumption of carbon per ton of pig-iron has been at the same time ascertained, then we are at once able to refer the case to its proper position; and the knowledge of the ratio between the two cases enables us at once to see what amount of transfer has been going on, and what prospect there is of effecting an improvement. It was further pointed out that the main causes of this injurious re-conversion of CO_2 into CO were (1) the fact that the limestone, used as flux, contains a proportion of CO_2, which can only be evolved at a red heat, and therefore in contact with red-hot coke, which immediately gives up some of its C to the evolved gases; (2) the fact that the ore, especially in the larger pieces, does not get completely de-oxidised until it reaches the red-hot region, where the CO ascending in the furnace first unites with the oxygen in the ore to form CO_2, and then absorbs another equivalent of C from the coke, so returning again to the condition of CO. It is therefore suggested that both these sources of evil might be removed if (1) the limestone were *calcined* before entering the furnace, so as to have already parted with its oxygen, (2) the ironstone were broken up into pieces small enough to insure their decomposition in the higher parts of the furnace. Another means of accomplishing the latter result was to increase still further, if necessary, the height of furnaces. A sanguine estimate was made of the economy that would attend the application of these two devices, which it was expected might reach over 3 cwt. of coke per ton of pig-iron made.

The value to ironmasters of the elaborate tables annexed to the paper, and of the mode in which the problem of blast-furnace economy is presented, cannot but be very great; but grave doubts were expressed in the discussion, by Mr. I. Lowthian Bell, F.R.S., whether the practical results would answer the author's expectations. As regards the use of calcined limestone, in particular, it was stated that it had already been tried, without effecting any economy, at least in large furnaces; the suggested reason being that the calcined lime, as soon as charged, re-absorbed CO_2 from the escaping gases, and that although heat was no doubt disengaged in the process, yet this was too near the throat of the furnace to have any serious effect. Moreover it is to be remembered that the previous calcining of the limestone must itself require fuel, the amount of which must be deducted from any apparent gain due to the absence of CO_2 in the lime within the furnace.

The fourth and last paper which was read (one on Screw Shafts, by Prof. Greenhill, of Woolwich, being postponed for want of time) was by a Swiss engineer, Herr Wendelstein of Lucerne, and gave a good and clear account of the mechanical arrangements connected with the construction of the great tunnel under the St. Gothard, the longest in the world. These are beyond our scope; and the important questions of temperature and ventilation, though just touched upon, were reserved for a future communication, which will also deal with the railway approaches. It may be mentioned that the observations of Dr. Stapff, the official geologist at the St. Gothard, were stated to give as the rate of increase of heat in that locality, 2° C. per 100 metres depth (1·1 Fahr. per 100 feet); and that, if this figure be applied to the Simplon tunnel, as at present proposed to be made just above Brieg, the heat to be dealt with would reach the very high figure of 47° C., or 116° Fahr.

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